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THE MESOZOIC FLORA OF PORTUGAL COMPARED WITH THAT OF THE UNITED STATES.

HISTORICAL NOTICE.

THE earliest studies in the Mesozoic deposits of Portugal seem to have been made by Mr. Daniel Sharpe, who read a paper before the Geological Society of London on April 11, 1832, describing certain beds in the vicinity of Lisbon and Oporto; in the former of which were included strata referred by him to the Oolite. On the 9th and 23d of January, 1839, he presented a second paper describing more fully the secondary formations in the vicinity of Lisbon.* On November 21, 1849, Mr. Sharpe read still a third paper before the same society† of a much more extended nature and devoted entirely to the secondary formation. In this paper is a full list of all the fossils known down to that date carefully determined by Mr. John Morris. Included in these was a single fossil plant regarded by Mr. Morris as a variety of a species of the Yorkshire Oolite called by Phillips *Cycadites gramineus.* It was found at Cape Mondego, and from this circumstance was given the varietal name *Mundæ.* As Mr. Morris referred Phillips' plant to the genus Zamites, the Portuguese plant was made to bear the name *Zamites gramineus* var. *Mundæ.*

In 1858 Sr. Charles Ribeiro published a series of elaborate papers on the Geology of Portugal,‡ treating chiefly of the Carboniferous; but in two of these§ he considers the Lias and Oolite, mentioning the plant above referred to from Cape Mondego and

* *Geol. Soc. Lond., Proc.*, Vol. I., p. 395; Vol. II., p. 31; Trans., 2d Ser., Vol. VI., p. 115ff.

† *Quart. Journ. Geol. Soc. Lond.*, Vol. VI., pp. 135–201.

‡ *Mem. Acad. Real. Sci. de Lisboa*, New Ser., Vol. II.

§ *Mina de Carvão de Pedra do Cabo Mondego, do districto de Leiria;* op. cit., Pt. II., Third and Fourth Memoirs (these memoirs are separately paged).

four other species from this and other localities.

Meantime other collections were being made, and in 1880 M. Paul Choffat published a somewhat elaborate report on the geology of the Jurassic of Portugal* in which the fossil plants were considered as far as available. The collections were sent by Choffat to Professor Oswald Heer, and a preliminary report upon them was received in time to be inserted as an Addendum. Heer's full report appeared a year later† and constitutes the first important contribution to the Mesozoic flora of Portugal. It also includes a large number of Tertiary plants. The horizons are here regarded as embracing: first, the Rhetic; second, the Jurassic, subdivided into Lias, Oolite or Dogger, and Upper Jurassic or Malm; and third, the Cretaceous, which was largely compared with the Wealden of other parts of Europe. Heer found in these collections 5 Rhetic, 18 Jurassic, and 23 Cretaceous forms. The Cretaceous plants consisted chiefly of ferns, cycads and conifers, but two of them were referred to the monocotyledons. No traces of dicotyledons were discovered.

M. Choffat continued his investigations and after Heer's death sent the plant-impressions to the Marquis Saporta at Aix; the latter was greatly interested in them and published three preliminary reports.‡ What specially attracted him was the presence of certain peculiar forms from this Lower Cretaceous horizon that he regarded as prototypes of the existing dicotyledonous flora. No dicotyledons had thus far been reported from any Lower Cretaceous deposit in Europe, and it had long been supposed that the Cenomanian was the earliest horizon at which this type existed. The several instalments embraced in these papers were from horizons in the Cretaceous, some of which were the same as those containing the plants described by Heer, while others were considerably higher. They contained a number of very remarkable forms, and the Marquis could not doubt that they represented ancestral dicotyledons. The full report upon these interesting collections has been waited for with great impatience, especially by American geologists familiar with our Potomac formation, in which the case is so nearly paralleled. In fact the present writer, having learned through correspondence with the Marquis that large collections were in his hands, and not knowing how soon his report would appear, was so desirous of learning more in regard to them that while in Europe during the past summer, by previous arrangement with him and at his urgent request, he paid a visit to the veteran paleobotanist at Aix, in the South of France, and through his extreme courtesy was not only permitted to examine these collections, but enjoyed the great favor of discussing with him a large number of the most interesting questions to which they give rise. It was then that he learned that the final report was already in press and would soon appear, and proof sheets of the text and plates were then in the possession of the author, so that it was possible to examine the work in immediate connection with the specimens. This work has now appeared* and copies of it are in the hands of American geologists; but it may

* *Étude stratigraphique et Paléontologique des terrains Jurassiques du Portugal.* Première livraison. Le Lias et le Dogger au Nord du Tage. Section des travaux géologiques du Portugal, Lisbonne, 1880.

† *Contributions à la flore fossile du Portugal par le* Dr. Oswald Heer. Section des travaux géologiques du Portugal, Lisbonne, 1881.

‡ *Comptes Rendus Acad. Sci. de Paris,* Vol. CVI., May 28, 1888, pp. 1500–1504; CXI., December 1, 1890, pp. 812–815; CXIII., August 3, 1891, pp. 249–253.

* *Flore fossile du Portugal.* Nouvelles contributions à la flore Mésozoique par le Marquis Saporta. Accompagnées d' une notice stratigraphique par Paul Choffat. (Avec 40 planches.) Direction des travaux géologiques du Portugal, Lisbonne, 1894.

as well be stated here that although a large and voluminous report containing 280 quarto pages and 39 plates, it still comes far short of covering the material that is now in the author's hands. The collections were sent to him in instalments almost every year and are still arriving, but it was necessary to fix some limit to the publication, which was closed at a certain date and the work sent to press, since which time other collections have been received, which were also carefully examined on that occasion at the Chateau of Fonscolombe, the country residence of the Marquis, 16 kilometers north of Aix, and upon which he was at the time actively engaged. These will be reported upon in a subsequent memoir. The remarkable parallelism between the plant bearing deposits of the west coast of Portugal and those of the eastern part of the United States, and especially between the Lower Cretaceous of Portugal and our Potomac formation, gives an especial interest to this memoir.

THE JURASSIC FLORA.

IN America there is a decided time hiatus between the lowest Potomac beds and the next plant bearing horizon below, which is now regarded as belonging to the extreme Upper Triassic and as about the equivalent of the Keuper deposits of Lunz, in Austria.* In Portugal, on the contrary, there appear to be no plant bearing horizons in the Trias proper, but in the Jurassic, which is absent in this country, a considerable number of such deposits have been found. M. Choffat, who prepared the geological part of this memoir, follows as closely as possible the nomenclature of the French geologists, and it is found that plant bearing horizons occur in the Infralias, part of which may be as low as the Rhetic, and some of which is referred to the Sinemurian; in the Lias; in several of the properly Oolitic beds (Toarcian, Bajocian, Callovian, etc.); in several members of the Corallian; in the Kimmeridgian, and in the Portlandian. The Jurassic deposits of Portugal consist of sandstones and limestones, the former predominating below; and while all of them may not be of marine origin, so large a part is fossiliferous that by the aid of the careful stratigraphical investigations of the Portuguese geologist it is possible to fix the position of the plant beds with relation to those holding animal remains, a fact which is of the utmost importance in determining the validity of the evidence of fossil plants in such countries as America, where, for the most part, no such guide exists.

The Jurassic flora of Portugal, as embraced in the present memoir and in that of Heer already mentioned, consists of 122 species, of which 22 are Infralias, 1 Lias, 8 Oolite, 8 Corallian and 88 Kimmeridgian. It is subdivided into 6 Algæ, 6 Equiseta, 70 ferns, 7 Cycads, 24 Conifers and 9 Monocotyledons. Of the ferns, which so largely predominate, 27 species belong to the genus Sphenopteris, 8 to Cladophlebis, 8 to Scleropteris, and 4 each to Pecopteris and Hymenophyllites. Of the conifers, which come next in importance, 5 belong to Pagiophyllum, 4 to Brachyphyllum, and 3 to Thuyites. The cycads belong to the two genera Podozamites and Otozamites. Seven of the Monocotyledons consist of small blades and culms of grasses, grouped under the genus Poacites.

A comparison of this Jurassic flora with that of the American Trias reveals the fact that while only 3 species, *Cheirolepis Münsteri*, *Pagiophyllum peregrinum* and *Palissya Brownii*, are common to the two, there are 14 genera that occur in both. In the number of species the two floras as now known are almost equal, that of the American Trias numbering 119, while that of the Portuguese Jurassic numbers 122. It is there-

*See *Bull. Geol. Soc. Am.*, Vol. III, 1891, p. 31.

fore important to note in what proportions these 14 genera occur in the two floras:

GENERA COMMON TO AMERICAN TRIAS AND JURASSIC OF PORTUGAL.

GENERA.	NUMBER OF SPECIES.	
	AMERICAN TRIAS.	JURASSIC OF PORTUGAL.
Baiera	3	1
Brachyphyllum	1 ?	5
Cheirolepis	2	1
Chondrites	3	1
Cladophlebis	7	8
Clathropteris	2	1
Equisetum	6	5
Otozamites	4	3
Pagiophyllum	6	5
Palissya	3	2
Pecopteris	1	4
Podozamites	2	3
Schizoneura	5	1
Voltzia	1	2

When we consider that the two horizons do not at all overlap and that more than three-fourths of the Portuguese plants come from the uppermost members of the Jurassic, it is not to be expected that the correspondence will be very close; and accordingly we not only miss in the Portuguese flora some of the largest American genera, such as Acrostichites, Ctenophyllum, and Pterophyllum, but also some of the most striking and abundant forms, such as Macrotæniopteris, while on the other hand no monocotyledons occur in the American Trias so far as known, and the two largest genera of ferns in the Portuguese Jurassic, Sphenopteris and Scleropteris, are entirely wanting in the American Trias.

THE CRETACEOUS FLORA.

THE Cretaceous flora of Portugal has much greater interest for the student of American paleobotany than the Jurassic flora, which has just been considered. First, because, as now known, it is considerably larger, numbering 199 species, but chiefly because we have in America a large number of plant bearing deposits that correspond so closely with those of Portugal that a comparison may be legitimately made that furnishes valuable results. It is true that our American Lower Cretaceous flora has now been so extensively worked that it has assumed relatively large proportions, numbering, so far as known, over 800 species. The Potomac formation alone furnishes no less than 737. The interest is still further heightened by the fact that in the Lower Cretaceous of both Portugal and America, the plant bearing beds occur at a number of distinct horizons, which may not without profit be directly compared in the two countries. For example, the Potomac formation now furnishes at least five distinct horizons from which fossil plants have been obtained, the lowest being that of the James River, which may extend as low as the top of the Jurassic. The next higher is that so well known at Fredericksburg, Virginia, and other points on the Rappahannock and Potomac Rivers. The third is the Mount Vernon clays which directly overlie the last named and have furnished a distinct flora. The fourth is well developed in the vicinity of Aquia Creek, the plant bearing beds near Brooke, Virginia. The fifth is undoubtedly much higher, and there appears to be a considerable thickness of non-fossiliferous deposits intervening between the last named and those plant bearing beds that have been discovered on the eastern side of the District of Columbia and at other points near Washington, on the Severn River, and on the Eastern Shore of the Chesapeake Bay, which have furnished a flora substantially identical with that of the Amboy clays on the Raritan River and of Staten Island, Long Island and Marthas Vineyard, as well as of the Tuscaloosa formation of Alabama.

The Lower Cretaceous of Portugal is subdivided into a very similar series of plant bearing deposits. One locality, Valle-de-Brouco, is referred by Choffat to the Infravalanginian, which is at the very base of the Neocomian and corresponds well with

our James River series. An important plant bearing locality between Matta and Valle-de-Lobos is regarded as Valanginian or Neocomian. It may be compared with the Fredericksburg beds of the Potomac formation. The beds of Almargem, which have furnished many species, overlie the recognized Urgonian and probably belong to the upper portion of that subdivision, or possibly to the base of the next one called by the French geologists the Aptian. It corresponds quite closely with the Kome beds of Greenland and may be compared with the Mount Vernon clays of the Potomac formation, though it is probably higher. Then there is a series of beds in the vicinity of Torres-Vedras, viz., at S. Sebastião, Quinta-da-Fonte-Nova, Forca, Quinta-do-Chafariz, Portella-da-Villa, etc., and another series in the vicinity of Cercal and Zambujeiro, which are classed as Aptian, between which and the last named there is a considerable interval, including marine deposits belonging to the Urgonian. Certain other beds, as at Caixaria and Caranguejeira, are less definitely fixed geologically, but probably belong to about the same horizon. The Aptian of the French geologists lies between the Urgonian below and the Albian above, and corresponds in the main with the lower Greensand of England. It may be compared with those deposits of the Potomac formation near Aquia Creek called the Brooke beds by Professor Fontaine, which have yielded a large number of fossil plants, including such well-marked dicotyledons as Celastrophyllum and Sapindophyllum.

Above these beds there is an abundant plant locality at Buarcos, which is classed as Albian, and still higher others at Nazareth, Alcanede and Monsanto, also regarded as Albian, but as belonging to that uppermost member called Vraconnian. The Albian corresponds in a general way with the Gault and is the uppermost section of the Lower Cretaceous, the overlying beds being Cenomanian, which is the lowest subdivision of the Upper Cretaceous. These Albian plant bearing beds may be roughly compared with what has been called in America the Amboy clays, but which has recently been more correctly named by Professor William B. Clark the Raritan formation. In America, as in Portugal, this deposit may also be divided into two parts, a lower and an upper, the former consisting of the beds along the Raritan, which themselves have a considerable thickness and show marked changes in the flora, while to the latter belong the deposits on Staten Island, Long Island and Martha's Vineyard, which have yielded large collections chiefly from indurated nodules formed in red clay.

Finally, in the Valley of Alcantara, at Padro, Pombal and Villa-Verde-de-Tentugal, there are plant bearing beds belonging to the Cenomanian. It is possible that these latter may not be higher than those of Long Island and Gay Head.

The floras of the several horizons in the Lower Cretaceous of Portugal differ less in their abundance than those of the Jurassic; the largest is that of the Valanginian, amounting to 86 species or over 43 per cent.; the Urgonian has yielded only 25 species or 12 per cent., the Aptian 42 species or a little more than 21 per cent., the Lower Albian 58 species or over 25 per cent., and the Upper Albian or Vraconnian 28 species or 14 per cent. The striking coincidence of the parallelism between these horizons and those of the Potomac formation in America is still further heightened by the circumstance, accidental perhaps, that the numerical proportion existing between the species now known at the corresponding horizons in America is very nearly the same. The Basal Potomac, corresponding to the Vraconnian, has yielded 329 species or a little over 44 per cent.; the Mount Vernon clays, which were compared with the Urgonian, 42 species

or somewhat less than 6 per cent.; the Aquia Creek beds, corresponding to the Aptian, 137 species or rather more than 18 per cent.; the Raritan beds and their equivalents, compared to the lower Albian, 264 species or nearly 36 per cent.; and the uppermost beds of Marthas Vineyard, Long Island and Staten Island, which may be called the Island Series and compared to the Vraconnian, 133 species or 18 per cent. These results may be put in the following tabular form:

LOWER CRETACEOUS OF PORTUGAL.		POTOMAC FORMATION OF THE UNITED STATES.	
HORIZONS.	PER CENT.	HORIZONS.	PER CENT.
Vraconnian . . .	14	Island series . . .	18
Lower Albian . .	29	Amboy Clays, etc.	36
Aptian	21	Aquia Creek (Brooke) Series.	18
Urgonian	12	Mt. Vernon Clays.	6
Neocomian	43	James and Rappahannock Series.	44

It will be remembered that the Mount Vernon clays have been very little developed as yet, and when this florula is thoroughly known it will probably fully equal that of the Almargem beds of Portugal, relatively to the total Potomac flora.

Taking the Cretaceous flora of Portugal as a whole, exclusive of the Cenomanian, it is found to consist of 4 algæ, 1 species of Isoetes, 3 of Lycopodites, 1 of Equisetum, 80 of ferns, 15 of cycads, 26 of conifers, 4 of anomalous types, classed by the author under the head of Proangiosperms, 18 of monocotyledons, 41 of dicotyledons, and 6 of forms of uncertain affinity.

It will be seen that as in the Jurassic, so in the Cretaceous the ferns predominate; and of these, 32 species belong to the genus Sphenopteris and 10 to Cladophlebis; 7 of the cycads belong to the genus Podozamites, and 3 to Glossozamites. The conifers are much more evenly distributed, there being 4 species of Brachyphyllum, and 3 each of Sphenolepidium and Thuyites, while a large number of genera have only one or two species; among these are Abietites, Baiera, Cheirolepis, Frenelopsis, Pagiophyllum, Palæocyparis, Palæolepis, Sequoia and Widdringtonites. The genera referred to the Proangiosperms are Changarniera, Eolirion, Yuccites, Delgadopsis and Protorhipis, some of which will require special mention further on. Half of the monocotyledons consist of grass-like objects referred to poacites, some of which he classes under the Proangiosperms, and others as true monocotyledons. The dicotyledonous flora is here well developed, but most of the forms occur in the Albian. Seven species are referred to a new genus, Proteophyllum, a name too near Protophyllum of Lesquereux, and Proteæphyllum of Fontaine, but the forms are different from both these; 4 to the new genus Dicotylophyllum, and 3 each to Eucalyptus and Salix.

In comparing the Cretaceous flora of Portugal with that of America it is true that we only find a few species that are common to the two countries, really only five, as follows:

Pecopteris Brauniana Dunk.

Sphenolepidium Kurrianum (Dunk.) Heer.

Sphenolepidium Sternbergianum (Dunk.) Heer.

Sphenopteris Mantelli Brongn.

Sphenopteris valdensis Heer,

the last of which only occurs doubtfully in the Trinity of Texas.

Add to these *Sequoir subulata*, of which a very near variety *lusitanica*, has been found in the Portuguese beds.

We should not, of course, expect the species to be common to any great extent, and the comparison is practically limited to the genera. Looked at from this point of view, we see that the resemblance is indeed close, a great number of the important genera occurring in both floras. There are no less than 46 of these common to the two,

though in some cases the author's individuality is probably alone responsible for slight differences of termination in the names. For example, forms referred to Baiera by one would be referred to Baieriopsis by the other, and so with Ctenis and Ctenidium, Myrsine and Myrsinophyllum, Oleandra and Oleandridium, Salix and Saliciphyllum, Thuya and Thuyites, etc.

Many of these genera, when we consider the difference in the size of the two floras, occur in both countries in nearly the same proportion. For example, of Aralia we have in Portugal 2 species, in America 11; of Brachyphyllum, in Portugal 4, in America 9; of Cladophlebis, in Portugal 10, in America 25; of Frenelopsis, in Portugal 2, in America 6; of Laurus, in Portugal 2, in America 8; of Myrica, in Portugal 2, in America 11; of Podozamites, in Portugal 7, in America 15; of Sphenolepidium, in Portugal 3, in America 9, etc. There are, of course, some cases in which the proportion is not the same. Thus, only one species of Magnolia occurs in the Portuguese beds, while in America we have 12, and on the other hand the largest Portuguese genus, Sphenopteris, represented there by 32 species, counts in America only 8 species. But here it may be supposed that the true representative in America of the Sphenopteris type of Portugal is really that exceedingly abundant genus Thyrsopteris, which numbers 40 species in the American beds. This would restore the relative proportions. On the whole, then, it may be considered that the Lower Cretaceous flora of Portugal is botanically speaking a very close repetition of that of America; and in view of the fact that in both countries a number of distinct horizons showing the progressive change in the flora throughout that period have yielded fossil plants in such a way that each of these florules may also be compared, the interest in the subject is almost fascinating.

ARCHETYPAL ANGIOSPERMS.

SPACE will only permit the consideration of one other important aspect, viz., a comparison of the dicotyledonous forms in the two countries, together with those ancestral types which the Marquis Saporta regards as prophetic of that great group of plants. This last question may be considered first. He finds among the specimens certain forms which he refers to the genus Protorhipis of Andræ. This genus was founded in 1855 upon some remarkable forms from the Lias of Steierdorf in Banat, Hungary,* which Andræ regarded as a fern and placed under the Pecopterideæ. He compares it with Jeanpaulia, which has since been proved identical with Baiera and correctly referred to the Coniferæ; also to Cyclopteris, Comptopteris, Diplodictyum, and Thaumatopteris, among fossils, and to Platycerium, among living ferns.

When I first saw the figure of his *Protorhipis Buchii*, I had grave doubts of its being a fern and fully believed that it represented some higher type of vegetation. I am, therefore, not surprised that the Marquis Saporta has arrived at the same conclusion, and am highly gratified that he has had the courage to give it publicity, notwithstanding the fact that Schimper, Schenk, Heer and Nathorst have all been content to regard it as a fern of the type of Drynaria, Platycerium, Allosorus, Clathropteris and the other living and fossil forms already mentioned.

In 1865 Zigno discovered another species, which, however, differs in a marked manner from the original of Andræ, having the margin entire. It is a small, deeply kidney-shaped leaf resembling that of some species of Asarum and was named *P. asarifolia*. This comes from the Oolite of Italy.†

*Lias-Flora von Steierdorf im Banate, by C. J. Andræ, Abhandl. geol. Reichsanst., Vol. II., Abth. 3, No. 4, 1855, pp. 35–36, pl. viii., fig. 1.

† Fl. Foss. Form. Oolithicæ, Vol. I, 1865, p. 180, pl. ix., fig. 2, 2a.

The forms described by Nathorst in 1878,* though much smaller are otherwise similar to *P. Buchii*, and Nathorst at first proposed to refer one of them to that species, but later concluded that it was distinct and made two species, *P. integrifolia* and *P. crenata*.

In 1880 Heer described another small cordate form from the Oolite of Siberia. It is similar to Zigno's species and was named *P. reniformis*.† Two years later, however, he found another similar form in the Kome beds, Urgonian, which is rather cordate than reniform and which he called *P. cordata*.‡ Both these forms have the margin entire.

Saporta in this work has revised all these forms and comes to the conclusion that they cannot be ferns, and although the original *P. Buchii* and both of Nathorst's species so closely resemble dicotyledonous leaves and are somewhat comparable in nervation to Credneria and some fossil Viburnums, as well as to such living genera as Glechoma and Chrysosplenium, still he hesitates to class them in that group. He has carefully refigured both of Nathorst's specimens, and also one that Nathorst figured without naming but regarded as probably a monocotyledon, but which Saporata considers to belong to the same type and calls *P. Nathorstii*. And these he carefully compares with the Portuguese form which he names *P. Choffati*, and classes the whole in special group which he long ago created and denominated the Proangiosperms, as representing the forerunners of both the monocotyledons and dicotyledons. The Portuguese species comes from Cercal, which Choffat places in the Aptian; it is therefore probably somewhat higher than the Kome beds of Greenland from which Heer derives one of his species; all the others, of course, are of far more ancient origin, viz., Jurassic, and it is not to be wondered at that no one should have ventured to refer them to any modern type.

*Fl. Bjuf. Heft 1, p. 42; Heft 2, p. 57, pl. ix., figs. 2, 4.

†Fl. Foss. Arct., Vol. VI., Abth. 1, Pt. 1, p. 8, pl. 1, fig. 4a.

‡Ibid., Abth. 2, p. 11, pl. iii, fig. 11.

Of the other four genera referred to this group, viz., Changarniera, Yuccites, Delgadopsis and Eolirion, the first two come from the Valanginian (Neocomian) of S. Sebatião, the third from the Aptian of Cercal, and the last from the Albian of Buarcos. They all seem to be ancestral monocotyledons. Delgadopsis occurs in two forms: first, as a sort of culm or broad striate stem; and secondly, in the form of a jointed rhizome, the swollen joints emitting innumerable rootlets, which, when absent, leave peculiar scars.

Choffatia Francheti, regarded by the author as a dicotyledon, is also a very remarkable plant, and has been aptly compared by him to certain euphorbiaceous forms, such as Phyllanthus. It also resembles some species of Euphorbia. It seems to be a floating aquatic, and specimens with the fibrous roots occur in the collection. In some of these descending fibers occupy one side of the stem or rachis, while the floating or aerial leaves occupy the other.

Upon the whole, it cannot be said that any of these higher types, found below the Albian, and corresponding in age to our middle and older Potomac, very closely resemble the plants of the same general class from the American beds of that age, and yet there are certain Potomac forms referred by Professor Fontaine to Menispermites, Hederæphyllum, Proteæphyllum and Populophyllum, whose areolate nervation somewhat resembles that of *Protorhipis Choffati*. The new genus Dicotylophyllum, of which he finds four species in the Aptian of Cercal, and which he very properly regards as a true dicotyledon, somewhat resembles the Protorhipis, but lacks the peculiar areolate

nervation. These leaves are all quite small, but show a somewhat distinct midrib, and usually 2–4 lateral primaries. In form they recall some species of Vitis or Cissities, and *D. cerciforme*, while not resembling Cercis, as the specific name would imply, has many of the characteristics of Hedera. It may be roughly compared with Professor Fontaine's Vitiphyllum from the Potomac of Baltimore, and except in size *D. hederaceum* and *D. corrugatum* are fairly comparable with *Populophyllum reniforme* (cf. Fl. Pot., pl. clvi., f. 3).

In the Albian beds of Buarcos, and especially in the Vraconnian of Nazareth, we begin to find some of the higher types. But the genus Proteophyllum has still a very ancient appearance with a more or less areolate nervation. It is a narrowly lobed leaf, remotely recalling in its general form some species of Dewalquea. It may be possible to trace this form into his *Aralia calomorpha* from the same beds. His *Adoxa prœatavia* is a very peculiar plant, which also reminds one of Vitiphyllum Font., although none of the species of the latter genus which show the branching character have yet been figured. His *Braseniopsis venulosa* has some of the characteristics of Protophyllum of Lesquereux, but is usually smaller and always entire; the nervation is also different, except at the base of the leaf, which has a large expansion below the summit of the petiole, as in Protophyllum. *Myrsinophyllum revisendum* will doubtles have to be revised. It is much like Potomac forms that have been referred to Myrica (e. g., *M. brookensis*) and Celastrophyllum. It is entirely different from the *Myrsine borealis* of Heer, which, with two other species, occur in the Amboy clays and Tuscaloosa formation. His *Geranium lucidum* is an exceedingly definite and handsome form, but it is hard to separate it generically from his *Cissites sinuosus*, and all of these seem to be analogous to our Vitiphyllum. His *Menispermites cercidifolius*, though much smaller, is not unlike Professor Fontaine's *M. Virginiensis*, especially the smaller forms which I have found in the Mt. Vernon clays. His *Aralia proxima* can scarcely be distinguished from *M. Wellingtoniana* of the Dakota group, more common in the Newer Potomac.

It is only in the Nazareth beds (Vraconnian) that we find the typical Amboy Clay flora. Here we have the Eucalyptus, Laurus (Laurophyllum), Salix, Myrsinophyllum, Sapindophyllum, etc., some of which are probably specifically identical with forms described by Newberry, and it is altogether probable that if the posthumous work of Dr. Newberry, now in press, had been in the hands of the present author a large number of the species would have been identified with American forms.

I will only notice one other significant fact. In the Cenomanian beds which overlie these last, as it would seem unconformably, but which may not be so widely separated from them as has been supposed, there occurs a large elongated leaf which the Marquis has called *Chondrophyton laceratum*. It agrees only in its finer nervation with *C. dissectum* Sap. and Mar., the only other species.* It has a very delicate nervation with small polygonal meshes, and an entire paryphodrome margin, but the remarkable fact is that it seems to have a deeply retuse summit. It is evident that from the specimen the author was unable to make this latter out with certainty; but he has drawn the marginal lines so as distinctly to indicate it. So desirous was he that this leaf should be correctly represented that he has given us two interpretations from drawings made at different times, figs. 4, 5 of pl. xxxviii. He states that he considers figure 5 to represent the form better than figure 4; and it is in this

*L. Évolution du Règne Végétal. Par Saporta et Marion. Les Phanérogames, Vol. II., Paris, 1885, p. 120, fig. 126.

that the terminal lobation is most clearly shown. A comparison of this figure with the numerous specimens of *Liriodendropsis simplex* of Newberry leaves no doubt whatever that the Portuguese plant is at least a congener of the American plant, and it is just possible that it may belong to the same species. As this form has been three times published* it is a little surprising that Saporta did not think to compare it with the Portuguese plant. There are differences in the finer nervation, but this is also perceptible between his two drawings of the same specimen; these also differ in different specimens of the American plant, and one or two other species remain to be published. When all the material is illustrated most of these differences will disappear. If any remain it can be ascribed to difference of age and geographical position.

LESTER F. WARD.

WASHINGTON.

EXPLANATION OF ACQUIRED IMMUNITY FROM INFECTIOUS DISEASES.†

IT has long been known that, in a considerable number of infectious diseases, a single attack, however mild, affords protection against subsequent attacks of the same disease; that in some cases this protection appears to be permanent, lasting during the life of the individual; that in others it is more or less temporary, as shown by the occurrence of a subsequent attack.

The protection afforded by a single attack not only differs in different diseases, but in the same disease varies greatly in different individuals. Thus certain individuals have been known to suffer several attacks of small-pox or of scarlet fever, although, as a rule, a single attack is protective. Exceptional susceptibility or insusceptibility may be not only an individual but a family characteristic, or it may belong to a particular race.

In those diseases in which second attacks are not infrequent, as, for example, in pneumonia, in influenza or in Asiatic cholera, it is difficult to judge from clinical experience whether a first attack exerts any protective influence. But from experiments upon the lower animals, we are led to believe that a certain degree of immunity, lasting for a longer or shorter time, is afforded by an attack of pneumonia or of cholera, and probably of all infections due to bacterial parasites. In the malarial fevers, which are due to a parasite of a different class, one attack affords no protection, but rather predisposes to a subsequent attack.

In those diseases in which a single attack is generally recognized as being protective, exceptional cases occur in which subsequent attacks are developed as a result of unusual susceptibility or exposure under circumstances especially favorable to infection. Maiselis has recently (1894) gone through the literature accessible to him for the purpose of determining the frequency with which second attacks occur in the various diseases below mentioned. The result is as follows:

	Second Attacks.	Third Attacks.	Fourth Attacks.	Total.
Small-pox . .	505	9	0	514
Scarlet fever .	29	4	0	33
Measles . . .	36	1	0	37
Typhoid fever .	202	5	1	208
Cholera . . .	29	3	2	34

Recent researches indicate that the principal factor in the production of acquired immunity is the presence, in the blood of the immune animal, of some substance capable of neutralizing the toxic products of the particular pathogenic microörganism

* Bull. Torr. Bot. Club, Vol. XIV., New York, Jan. 1887, p. 6, pl. lxii, figs. 2, 3, 4; Am. Journ. Sci., Vol. XXXIX., New Haven, February, 1890, p. 98, pl. ii., figs. 6, 7; Trans. N. Y. Acad, Sci., Vol. XI., 1892, p. 102, pl. ii., figs. 2–7, 9.

† Abstract of a paper read before the Biological Society of Washington, March 9, 1895.

against which immunity exists, or of destroying the germ itself.

The substances which destroy the toxic products of pathogenic bacteria are called antitoxins. As pointed out by Buchner in a recent paper, the antitoxins differ essentially from the so-called alexins, to which natural immunity is ascribed. The alexins are characterized by their germicidal and globulicidal action—they destroy both the red corpuscles and the leucocytes of animals belonging to a different species from that from which they have been obtained, and by their coagulability and instability—destroyed by sunlight and by a temperature of 50° to 55° C. On the other hand, the antitoxins best known (diphtheria and tetanus) have no germicidal or globulicidal action; they resist the action of sunlight and require a temperature of 70° to 80° C. for their destruction.

Our knowledge of the antitoxins dates from the experiments made in the Hygienic Institute of Tokio, by Ogata and Jasuhara, in 1890. These bacteriologists discovered the important fact that the blood of an animal immune against anthrax contains some substance which neutralizes the toxic products of the anthrax bacillus.

In the same year (1890) Behring and Kitasato discovered that the blood of an animal which has an acquired immunity against tetanus or diphtheria, when added to a virulent culture of one or the other of these bacilli, neutralizes the pathogenic power of such cultures, as shown by inoculation into susceptible animals. And also that cultures from which the bacilli have been removed by filtration, and which kill susceptible animals in very small amounts, have their toxic potency destroyed by adding to them the blood of an immune animal, which is thus directly proved to contain an antitoxin which comparative experiments show not to be present in the blood of non-immune animals.

During the past two or three years numerous additional experiments have been reported which confirm the results already referred to, and show that immunity may be produced in a similar manner against the toxic products of various other pathogenic bacteria—the typhoid bacillus, the 'colon bacillus,' streptococcus pyogenes, staphylococcus pyogenes aureus and albus, etc.

The Italian investigators, Tizzoni and Centanni, in 1892, published a preliminary communication in which they gave the results of experiments which appear to show that in guinea-pigs treated with tuberculin, by Koch's method, a substance is developed which neutralizes the pathogenic potency of the tubercle bacillus. Professor Tizzoni and his associate, Dr. Schwarz, have also (1892) obtained evidence that there is an antitoxin of rabies. Blood-serum taken from a rabbit having an artificial immunity against this disease was found to neutralize, *in vitro*, the virulence of the spinal marrow of a rabid animal after a contact of five hours.

Professor Ehrlich, of Berlin, in 1891, published the results of some researches which have an important bearing upon the explanation of acquired immunity, and which show that susceptible animals may be made immune against the action of certain toxic proteids of vegetable origin, other than those produced by bacteria; also that this immunity depends upon the presence of an antitoxin in the blood-serum of the immune animals.

The experiments of Ehrlich were made with two very potent toxalbumins—one ricin, from the castor-oil bean; the other, abrin, from the jequirity bean. The toxic potency of ricin is somewhat greater than that of abrin, and it is estimated by Ehrlich that 1 gm. of this substance would suffice to kill one and a half million of guinea-pigs. When injected beneath the skin in dilute solution it produces intense local inflammation, resulting in necrosis. Mice are

less susceptible than guinea-pigs, and are more easily made immune. This is most readily accomplished by giving them small and gradually increasing doses with their food. As a result of this treatment the animal resists subcutaneous injections of 200 to 300 times the fatal dose for animals not having this artificial immunity.

Ehrlich gives the following explanation of the remarkable degree of immunity established in his experiments by the method mentioned:

"All of these phenomena depend, as may easily be shown, upon the fact that the blood contains a body—antiabrin—which completely neutralizes the action of the abrin, probably by destroying this body."

In a later paper (1892) Ehrlich has given an account of subsequent experiments which show that the young of mice which have an acquired immunity for these vegetable toxalbumins may acquire immunity from the ingestion of their mother's milk; and also that immunity from tetanus may be acquired in a brief time by young mice through their mother's milk.

A most interesting question presents itself in connection with the discovery of the antitoxins. Does the animal which is immune from the toxic action of any particular toxalbumin also have an immunity for other toxic proteids of the same class? The experimental evidence on record indicates that it does not. In Ehrlich's experiments with ricin and abrin he ascertained that an animal which had been made immune against one of these substances was quite as susceptible to the toxic action of the other as if it did not possess this immunity, *i. e.*, the anti-toxin of ricin does not destroy abrin, and *vice versa*.

We have also experimental evidence that animals may acquire a certain degree of immunity from the toxic action of the venom of the rattlesnake. This was first demonstrated by Sewall (1887), and has been recently confirmed by Calmette (1894). In his paper detailing the results of his experiments the author last named says:

"Animals may be immunized against the venom of serpents either by means of repeated injections of doses at first feeble and progressively stronger, or by means of successive injections of venom mixed with certain chemical substances, among which I mention especially chloride of gold and the hypochlorites of lime or soda.

"The serum of animals thus treated is at the same time preventive, antitoxic and therapeutic, exactly as is that of animals immunized against diphtheria or tetanus.

"If we inoculate a certain number of rabbits, under the skin of the thigh, with the same dose, 1 millgr. of cobra venom, for example, and, if we treat all of these animals, with the exception of some for control, by subcutaneous or intraperitoneal injections of the serum of rabbits immunized against four millgrs. of the same venom, all of the control animals not treated will die within three or four hours, while all of the animals will recover which receive 5 c. c. of the therapeutic serum within an hour after receiving the venom."

As a rule the antitoxins have no bactericidal action; but it has been shown, by the experiments of Gamaleïa, Pfeiffer and others, that in animals which have an acquired immunity against the spirillum of Asiatic cholera and against spirillum Metchnikovi there is a decided increase in the bactericidal power of the blood-serum, and that immunity probably depends upon this fact.

Certain important questions present themselves in connection with the production of antitoxins and germicidal substances in the blood of immune animals, one of which is: Is the production of the antitoxin continuous while immunity lasts, or does it occur only during the modified attack which results from inoculation with an attenuated virus, or of filtered cultures, the antitoxin being subsequently retained in the circulating blood? The latter supposition does not appear very plausible, but it must be remembered that these antitoxins do not dialyze—*i. e.*, they do not pass through ani-

mal membranes—and consequently would not readily escape from the blood-vessels, notwithstanding the fact that they are held in solution in the circulating fluid. On the other hand, the passage of the tetanus antitoxin into the mother's milk would indicate a continuous supply, otherwise the immunity of the mother would soon be lost. Further experiments are required to settle this question in a definite manner, and also to determine the exact source of the antitoxins in the animal body and the *modus operandi* of their production.

GEO. M. STERNBERG.

WASHINGTON.

REMARKING THE MEXICAN BOUNDARY.

MR. A. T. MOSMAN, assistant in the U. S. Coast and Geodetic Survey, one of the commissioners on the part of the United States, presented an interesting summary of the work at a meeting of the National Geographic Society in Washington on the 8th inst.

At the initial meeting of the commissioners for the two countries, it was agreed that any of the old monuments recovered should be taken as defining the line; that new monuments should be interpolated between them, so that no two monuments should be more than 8000 metres apart, as required by the new treaty. The line had been marked under the treaty of 1853, by 52 monuments; the commissioners found 38 of these standing in 1891. On the parallels the new monuments mark the curve of the parallel, but on the oblique lines the monuments recovered were not accurately located on the line joining their extremities, and the boundary on these lines as now marked is, therefore, a broken line. Old monuments were recovered at all important points on the boundary, including all points where the line changed direction, but the distances between them were unequal, and in one instance exceeded 100 miles. The line from El Paso on the Rio Grande to San Diego on the Pacific, 700 miles, is now defined by 258 monuments.

The field work required the redetermination of the geographic positions of the old monuments recovered, and presents some interesting comparisons showing the facility and certainty of modern methods. The longitudes of the old monuments were determined by Emory from transits of the moon and moon culminating stars. In the relocation the longitudes were determined by the telegraphic method, connected with the geodetic work of the Coast Survey by coast survey parties working in conjunction with the commissioners. The greatest difference developed from Emory's positions was $4' 34''.3$ with other differences of $34''$ and $54''$ and still smaller quantities showing the old work to have been remarkably good for the method. The latitude stations in the new work were about 20 miles apart over the whole line, and at each station an azimuth was observed on Polaris near elongation to start the direction for the new tangent for the parallel and check the tangent ending at the station. The latitude observations were made with the zenith telescope formerly used on the N. W. boundary, but improved with new micrometer and levels. The telescope has a focal length of 826 mm., and the objective a clear diameter of 67 mm. A new departure was made in mounting the instrument on a wooden pier constructed in a simple form, readily transported. Its stability proved as great as a brick or stone cemented pier, as it was not uncommon to secure a whole night's work without relevelling, and the instrument invariably remained for several hours with level correction less than one div.$=1''.28$. The probable errors of the latitude determinations from the U. S. observers $= \pm\ 0''.03$ to $0''.4$. The Mexican observations have not yet been received. The plan of operations agreed upon required

independent determinations by the representatives of both governments. This was not practicable in the longitude determinations, but in the latitudes, running the parallels and locations of the numerous monuments, it was strictly carried out. The mean difference in the location of the 258 monuments, was less than three-tenths of a metre; the maximum difference was only 1.8 m., which occurred in locating a point about midway between two old monuments 100 miles apart, and over a very rough mountainous country, where the distances between water holes was over 60 miles. The angular variations of the lines run by the two parties at this point was a little more than three seconds.

The final results from the astronomical observations were required for immediate use on the ground; to permit the computations the mean declinations for the stars for latitude had been furnished by Professor T. H. Safford, of Amherst. In this way the latitude and azimuth were always available within three or four days after the observations were completed, a feature of such work that, it is believed, has not heretofore been attempted. Mr. Mosman promises that a list of the stars furnished by Professor Safford, some 600, will be published in the report of the commission, to be available for future work in the same latitude.

In locating the intermediate monuments the commission made use of the stadia, with gratifying results. On the parallel of 31° 47′ for a distance of 100 miles both chain and stadia were used for the purpose of comparison. It was found that the stadia was much more reliable than the chain, even on the desert, and in a rough country was much superior. The whole line was measured by both the American and Mexican engineers independently; when the two results for any distance differed more than one part in 500, remeasurements were made by steel tape or triangulation to discover the error. Many lines determined by triangulation were compared with the lengths determined by stadia, and the results showed that the stadia measurement could be relied on within one part in 1000. One line of 45 miles measured over rolling sand hills differed by one part in 1800 only.

In addition to the astronomical work, a strip of topography was surveyed on the American side $2\frac{1}{2}$ miles wide, and a line of levels was run with the wye level from the Rio Grande to San Diego, giving the elevation of each monument above mean tide of the Pacific Ocean. The levels were checked at Yuma with R. R. levels from San Francisco, showing the infinitismal discrepancy of two hundredths of a metre, probably an accident. At the Rio Grande there is a discrepancy of about two metres, but the datum plane for the R. R. levels at this place is not known.

O.

THE NATURE OF SCIENCE AND ITS RELATION TO PHILOSOPHY.

IF any one should ask me, 'What is physics?' I would tell him to study in the physical laboratory for ten years and then what he had learned by the time he was through would be the nearest he could get to an answer to the question. So to the question, 'What is science?' I can give no other general answer than that to anyone it is just what he knows about it. I can, however, give as a particular answer what I have in my own experience found science to be.

Science consists of weighing evidence and stamping each statement with an index of its reliability. That the sun moves around the earth is, according to the evidence at present produced, a statement with a reliability of 0. That the earth moves around the sun, we at the present day stamp as certain. That Mars contains living beings is to-day stamped as quite improbable. On the scale of probability where 0 means

not at all probable, and 1 means secure, $\frac{1}{2}$ means indifferent, we might say that such a statement regarding Mars would have a probability perhaps of $\frac{1}{25}$.

The difference between the unscientific and the scientific mind lies in the extent of evidence. The woman who lately left a fund for a prize to the one who shall establish communication with Mars had gathered enough evidence to give, in her mind, a high degree of probability to the supposition of the possibility of such an undertaking. And yet the members of the French Academy who accepted the money in the sense that it should go to the one making the best contribution to our knowledge of Mars were evidently in possession of enough further evidence to attach a very small degree of probability to the supposition.

This is the actual work of all the sciences. We cannot and dare not make statements except just so far as warranted by the facts. If you say that the act of discrimination increases the time of thought, the psychologist must answer yes, with a high degree of probability, because carefully collected experimental evidence points that way. If you say that consciousness is continuous during sleep, the psychologist must answer that reliable evidence is lacking, and that he is entitled to no opinion either way.

We often hear, from philosophers of the old school, the statement that the facts of the universe are divided into classes, each of which is given over to a science for investigation regarding details, while the general conclusions are reserved for the philosophers.

I must object to the limitation of science to the investigation of individual facts. Many of the problems with which a scientist is most directly concerned are the most general of all. The subject of time is one to which the psychologist and the astronomer devote their special attention. There can hardly be anything more general than the great independent variable, as it is called. Likewise space forms a problem for geometry, physics and psychology.

As every scientist knows, an investigator in one science is forced to learn a dozen other sciences; the more he specializes, the more remotely must he go for his information. For example, the specialist in experimental psychology is obliged to be more or less familiar with the science of measurement, with the astronomical determination of time, with portions of meteorology, with physics, with portions of organic chemistry and physical chemistry, with statistics, ethics, anthropology, etc., etc. The mediæval philosopher likes to bottle things up and label them, but the modern sciences are too lively specimens for that process.

This brings me to the question of the relation of science to philosophy. According to Wundt the work of philosophy is to take up and discuss the most general questions, time, space, number, etc., which cannot be handled by the particular sciences.

But let us consider a moment. Suppose the U. S. Government wishes a report on Lake Tahoe. It would go to the geographer to learn where it is, to the U. S. Survey to learn its measurements, to the chemist to know its composition, to the meteorologist to inquire about its weather, to the land owners for the price of land, to the boatman to learn the sailing qualities, etc., etc. It would print the reports all side by side for each reader to assimilate as he would or could. What it would not do would be to send out a special agent who should look into these matters himself and make his own report. We very well know that such agents filter through more of themselves than of the facts; they see what they bring eyes to see, and no one can be master of a dozen sciences or trades.

Suppose, however, it is desired to have a

treatment of the subject of 'time.' Wundt would propose that a special agent, called a philosopher, should gather up all he can from everybody and should present it as he thinks best. So with all the other fundamental questions. The result is that we have as many systems of philosophy as we have writers. Would it not be better to get the astronomer to present his experience with time, then the physicist to present his, then the psychologist, and so on? The reader can then assimilate what he is able, instead of accepting it as previously assimilated by the philosopher, as a kind of 'predigested' food.

A somewhat similar thought was spoken by Paulsen some years ago. I do not know if he has stated it in print. He considered that the day of philosophical systems was past; there could be text-books of philosophy as well as text-books of all sorts of things, but philosophy itself would consist of monographs by specialists.

Of course, on such conditions as these, we should be obliged to conclude that philosophy has no relation to the sciences and that, having the astronomer, the mathematician, the physicist, the geologist, the psychologist, the economist and all the others, we can entirely dispense with the philosopher.

E. W. SCRIPTURE.

YALE UNIVERSITY.

'SCIENCE.'

[THE following article, contributed by one of the original supporters of SCIENCE, will prove of interest to those who are not acquainted with the earlier history of the journal. All men of science are under very great obligations to Mr. Bell and Mr. Hubbard for establishing a weekly journal of science in America at a time when the conditions were less favorable than at present; to Mr. Scudder for the high standard maintained during his editorship, and to Mr. Hodges for his faithful and untiring efforts on behalf of the journal.

J. McK. C.]

IN 1882 Mr. A. Graham Bell conceived the idea of establishing a scientific journal, which should do for America what 'Nature' does for England. For this purpose, he was willing to contribute, with the coöperation of Mr. Gardiner G. Hubbard, the sum of twenty-five thousand dollars, which, in the estimation of good judges, would be sufficient to start a weekly paper and put it on a paying basis. Mr. Bell furnished the larger proportion of this sum. Mr. Samuel H. Scudder, of Cambridge, Mass., became the editor. President Gilman, of Johns Hopkins; Major Powell, of the Geological Survey; Professor Newcomb, of the Nautical Almanac; Professor O. C. Marsh, of New Haven; and Professor Trowbridge, of Columbia College, agreed to give their advice, and to act with Messrs. Bell, Hubbard and Scudder as a Board of Directors. This board, representing different interests and localities, possessed great weight with the entire community, and was believed to be generally acceptable to scientists.

The first number of 'SCIENCE' appeared February 9, 1883, some six or eight months subsequent to the conception of the idea. Mr. Moses King, the first publisher, retired the succeeding September. Shortly after, Mr. C. L. Condit, formerly with the 'Nation,' took charge of the publishing department and continued until the spring of 1886. Mr. Scudder retired from the editorship in 1885 and was succeeded by Mr. N. D. C. Hodges, when the office was removed from Cambridge to New York. It was soon found that twenty-five thousand dollars was not sufficient, and Messrs. Bell and Hubbard continued to advance further sums until, in 1886, they had expended about seventy-five thousand dollars, without having made the paper self-supporting.

An arrangement was then made with Mr.

Hodges to assume the entire charge of SCIENCE for a fixed annual sum. For three years M. Hodges had charge of the paper, under the advice of the Board of Directors. Mr. Hodges made large reduction in expenses of publication, but unfortunately made a larger reduction in the subscription price, from five dollars to three dollars and fifty cents a year.

It was never the intention of Messrs. Bell and Hubbard to make a profit from the publication of SCIENCE, but they did expect its establishment to make a contribution to science.

The circulation of the journal, under the management of Mr. Hodges, largely increased, and the changes made by him and his associate editors, Messrs. D. G. Brinton, of Philadelphia, and Charles Platt, of Baltimore, whose services were given gratuitously were of great value. It was originally supposed that advertisements would contribute largely to its support, but they were not obtained, partly on account of the limited circulation, and more largely because advertisers preferred to publish in special journals rather than in one intended to meet the wants of the scientific public.

The publication of SCIENCE was stopped for a time a year ago, although its circulation was then larger than it ever had been, the stringency of the times preventing many from paying their subscriptions.

At the meeting of the American Association for the Advancement of Science, at Brooklyn in 1894, the renewal of the publication of SCIENCE was brought before the Association. A large committee was chosen to consider its usefulness, and the propriety of contributing towards its support. Mr. Hodges appeared and stated fully his views and plans; the Association then voted that a contribution of fifteen hundred dollars should be made for the purpose of enabling Mr. Hodges to continue its publication. Immediately after Mr. Hodges decided that he could not continue the publication, and therefore this arrangement fell through.

Subsequently the reorganization of SCIENCE was undertaken by Professor Cattell, of Columbia College, who will, we trust, make it a success.

It would not be proper to close this article without an acknowledgment of the great ability, untiring zeal and never flagging interest shown by Mr. Hodges in his connection with SCIENCE.

CORRESPONDENCE.

A CATALOGUE OF SCIENTIFIC LITERATURE.

EDITOR OF SCIENCE:—The admirable plan for a card catalogue of scientific literature recommended to the Royal Society by the Harvard University Council (reprinted in the current volume of SCIENCE, pages 184–186) strongly commends itself to users of scientific literature, and has already been adopted with minor modification by at least one national scientific society. A slight extension of the plan in one respect would seem, however, to be advantageous.

The body of scientific literature is vast and constantly increasing, and scientific authorship and publication are rapidly extending from country to country and from point to point in each country throughout the world. Population is increasing, and with it writing and printing increase; civilization is spreading, and with it literature is expanding in an increasing ratio; science is becoming increasingly important as a directing and controlling force in civilization, and so the growth of scientific writing outstrips that of non-scientific scripture; the domain of science is widening rapidly as research concerning every conceivable subject pushes into and illumines the penumbra of half-knowledge, and thus the subject-matter of scientific literature is differentiated. Moreover, the fashion of scientific publication is changing; few recent investigators

spend years on a book, the masterpiece of a decade or a lifetime; most keep pace with the rapid progress of the times by issuing their chapters or sections as completed from time to time in the form of articles or brochures; and thus the average number of titles to be credited to individual authors is increasing. So the augmentation in scientific literature is many-branched and cumulative, and its rate is constantly augmenting. With the multiplication of scientific literature the need for comprehensive cataloguing is multiplied; yet with the multiplication the difficulty of measuring the teeming flood from the scientific press is increased in still larger measure. The task before the Royal Society is one of great magnitude.

It would seem that the success of the scheme for cataloguing scientific literature will depend largely on the intimacy of the relations to be established between the Royal Society, on the one hand, and (1) trade publishers, (2) non-commercial publishers, and (3) individual authors, on the other hand. Now, the basis for the relations between the central organization and trade publishers, and through them with the authors, is the simple one of financial interest; it is set forth in a satisfactory manner in the report of the University Council, who point out that it would be to the interest of the writers, as it would be also to that of the publishers, to prepare summaries suitable for carding by the central organization. In the case of this class of publishers, perhaps the leading interest would be that of the publishers themselves, who might accordingly be trusted to induce negligent authors to prepare the requisite summaries.

The non-commercial publishers include those issuing (a) periodicals put forth without hope of profit and often at individual sacrifice, which it would be useless to advertise in the ordinary way by reason of the limited number of possible subscribers; (b) proceedings, transactions and related serials published in limited editions by many scientific societies; (c) reports of official bureaus, like the U. S. Geological Survey and various State institutions, to whom increased distribution means no profit, but some loss in time, if not money; and (d) privately printed and irregularly published brochures, booklets and leaflets, commonly issued by the authors themselves. All of these classes of publications are important in this and several other countries; collectively, in this country at the present time, at least, they probably contain the major part of the material which should be catalogued by the Royal Society. To bring their contents within reach of a central organization would involve a wide-reaching and constant coöperation, which manifestly cannot be brought about through the ordinary financial stimulus, since the publication is not made on a commercial basis; it can be brought about, if at all, only through the inspiration of creative genius and authorial ambition. There are few scientific writers who would not be willing, indeed glad, to prepare summaries of their writings for the sake of securing wider publicity and more permanent record of their discoveries and ideas; for it is the laudable ambition for publicity and permanent record, for the good of men, that inspires the original writing, if not indeed the research itself. Many of the non-commercial publishers themselves are actuated by similar motives, and would be willing to incur the small tax of periodically sending summaries to the central organization, while others would doubtless be stimulated thereto by the authors themselves; yet, it is probable that so far as the non-commercial publications are concerned, the stronger bond of connection would be that between the central organization and the authors; and since the more natural relation is the hierarchic one, first

from central body to the less numerous class and from this in turn to the more numerous, any device that would strengthen the relation between the central body and the publishers would be useful. Thus, it might be well for the Royal Society to furnish sets of cards pertaining to the specialty represented by the non-commercial publication, either in exchange simply for the periodical transmission of summaries or in return for such summaries and for printing in the advertising pages or elsewhere a standing notice of the Royal Society catalogue. The coöperation of the publishers in securing, and indeed in editing, the summaries would be highly desirable, partly because with most writers summaries or abstracts need editorial scrutiny more sadly than their ordinary writing. It may be noted also that in these days of the making of many bibliographies there is a special need for abstracts and summaries for a wide variety of purposes, and the recognition of this need will make easier the way of the Royal Society in putting its plans into execution. Partly for this reason there would seem to be a certain desirability in printing the brief summaries, perhaps in a distinctive type, in conjunction with scientific articles.

The Geological Society of America recently concurred in a report to the Royal Society conforming to that of the Harvard University Council, with a brief addition designed to facilitate obtaining summaries of articles from non-commercial publishers of scientific literature, this addition having been suggested by the writer as one of the committee on the subject.

W J MCGEE.

TEACHING BOTANY ONE TOPIC AT A TIME, ILLUSTRATED BY SUITABLE MATERIALS AT ANY SEASON OF THE YEAR.

EDITOR OF SCIENCE—*Sir:* The recent papers in SCIENCE concerning the management of classes in botany prompt the following. In these times, of course, every true teacher of botany insists that his pupils shall study the objects before receiving much, if any, instruction from books or persons. I take it for granted that any teacher of a class beginning subjects that are treated in *Gray's Lessons* would prefer to take them up in about the sequence there given, but he will find it impossible to procure at any season of the year enough suitable material that is fresh to fully illustrate many of the sections of the book. For example, he cannot procure at any one time suitable materials to illustrate the section on stamens. The varieties there illustrated appear at different dates some weeks apart. So of the forms of pistils, the torus, fruits, etc. My plan has been to collect quantities of stamens of the barberry, sassafras, lobelia, cypripedium, mallow, locust, dandelion, lily, tulip tree, blueberry, sage, milkweed, and in most cases preserve each kind by itself in twenty-five per cent. alcohol, or in formalin one hundred of water to one of formalin. These are ready when we want to study stamens. A specimen or more of each kind of the preserved objects for illustrating any section of this subject can be placed in a small dish before each pupil in case fresh specimens cannot be procured. In many instances, when not allowed to dry, these can be gathered up and used for several successive classes.

In like manner, it is very satisfactory to be able, when fruits are to be studied, to have a good many kinds to illustrate the various sorts, such as half grown plums or cherries, the mandrake, bloodroot, violet, mulberry, winter-green, etc. Lessons in morphology can, in this way, be made more impressive than when some of the illustrations are used in one day and others in a week or a month.

W. J. BEAL.

AGRICULTURAL COLLEGE, MICH.

SCIENTIFIC LITERATURE.

Nicolái Ivánovich Lobachèvsky.—Address pronounced at the commemorative meeting of the Imperial University of Kasán, October 22, 1893, by Professor A. VASILIEV, President of the Physico-Mathematical Society of Kasan.—Translated from the Russian, with a preface, by DR. GEORGE BRUCE HALSTED, President of the Texas Academy of Science.—Volume one of the neomonic series.—Published at The Neomon, 2407 Guadalupe Street, Austin, Texas, U. S. A. 1894. Sm. 8vo, pp. 8+40+17.

Within the last thirty years the name of Lobachevsky has become widely known as that of one of the earliest discoverers in the field of non-Euclidean geometry, a subject which has not only revolutionized geometrical science, but has attracted the attention of physicists, psychologists and philosophers.

Professor Vasiliev's life of Lobachevsky, which we welcome here in an English translation, is without question the best and most authentic source of information on this original mathematical thinker who spent his whole life in a remote Russian town, almost on the confines of civilization, and whose work began to be appreciated by the scientific world only after his death (1856). What lends a peculiar interest to the story of this uneventful life is its intimate association with the growth of the University of Kazàn. Lobachevsky entered this university as a student soon after its foundation, became, immediately after graduation, an instructor, and then a professor in it, was its president for nineteen years during its formative period, and contributed largely to its rise and progress through his administrative ability and untiring energy. This man, who is known abroad as an original investigator in one of the most abstruse branches of mathematics, endeared himself, moreover, to his townsmen in many respects as a progressive and public-spirited citizen, delivering popular lectures on scientific subjects, conducting evening classes in elementary science for workingmen, taking a most active part in the work of the Kazàn Economic and Agricultural Society, and so on.

It is due to these facts that the centennial celebration held by the Physico-Mathematical Society of the University of Kazàn, in 1893, in commemoration of his birth, was participated in not only by professional mathematicians, but also by the whole university and the citizens of Kazàn. It is for this occasion that Professor Vasiliev prepared his biography.

The celebration began with religious services in the University chapel, on Lobachevsky's one hundredth birthday, November 3 (or, according to the old calendar still used in Russia, October 22); at noon the University Senate assembled in solemn session, the foreign delegates were greeted by the president of the university, letters and telegrams of congratulation were read, and several addresses were made commemorating the life and work of the great Russian geometer. On the next day the Physico-Mathematical Society held a public session for the reading of various papers on subjects connected with non-Euclidean geometry. On the 5th of November the Municipal Council of the city of Kazàn dedicated with appropriate ceremonies a memorial tablet, inserted in the front wall of the house in which Lobachevsky had lived. Another meeting of the Physico-Mathematical Society brought the celebration to a close. A sum of several thousand rubles had been collected in the course of the year for the purpose of founding a Lobachevsky medal or prize to be awarded annually, and of erecting a bust of Lobachevsky at Kazàn, in the public square that bears his name.

It is well that this late justice should be

done to the memory of a man who during his lifetime never received any public recognition for his scientific work. At the present time no competent mathematician doubts the value of Lobachevsky's investigations in non-Euclidean geometry. For those not familiar with modern mathematical thought it is, however, difficult, if not impossible, to fully appreciate the true value of this subject; they are inclined to attribute undue importance to its possible bearings on non-mathematical questions and to neglect and underrate what is most valuable.

The starting point for Lobachevsky's researches, as for those of all the earlier writers on non-Euclidean geometry (Saccheri, Lambert, the two Bolyais), is given by the theory of parallels in elementary plane geometry which is based by Euclid on his fifth postulate (usually called his "eleventh axiom"). This postulate refers to two lines cut by a transversal, and states that if the sum of the interior angles on one side of the transversal be less than two right angles the lines will meet on this side if sufficiently produced. The numerous attempts that have been made to make a theorem of this proposition, and to *prove* it, have always remained as futile as the attempts to square the circle. They have only shown that it can be replaced by other postulates, such as that only one parallel can be drawn to a given line through a given point, or that the sum of the angles of a triangle is equal to two right angles, etc.

Does it follow that these postulates express an absolute necessary truth? Certainly not. For it can be shown—and this is just what Lobachevsky did—that a perfectly consistent system of geometry can be constructed by rejecting Euclid's postulate and its equivalents, and assuming, say, that more than one parallel can be drawn to a given line through a given point, or that the sum of the angles of a triangle is less than two right angles.

The question of the character of the so-called geometrical axioms thus assumes an aspect very different from the one it had at the beginning of the present century, when they were commonly regarded as necessary logical truths. It is, however, not for the mathematician to decide whether ultimately these axioms express facts of observation unconsciously acquired and made familiar through the constant perception of an actually existing space. For him they represent mere assumptions selected for the purpose of defining his space or his methods of measuring this space.

It would, of course, be very important to know which of the different spaces that the mathematician can thus define corresponds most closely to the facts of observation. But this question is difficult to decide; for while the ordinary Euclidean space appears in this respect to satisfy all demands, the non-Euclidean spaces do the same, at least, approximately within certain limits; and all our observations give only approximate results and are confined within a narrow range of space.

What the mathematician has gained through the generalization of non-Euclidean geometry is a broader horizon and a vastly extended field of research. The multifarious relations by which this new science is connected with the various banches of geometry are admirably set forth by Professor F. Klein, of Göttingen, in his *Vorlesungen über nicht-Euklidische Geometrie* (1889–90). These lectures also trace the historical development of the subject since the times of Gauss. A few more recent investigations were discussed by him in the *Evanston Colloquium* (New York, Macmillan, 1894), in the 6th and 11th lectures.

What Professor Vasiliev tells us about Bartels, who in his earlier years had intimately associated with Gauss, and later, as the first professor of mathematics at the University of Kazàn, became the teacher

and protecting friend of Lobachevsky, confirms the supposition that the first impulse to these studies came to him, at least indirectly, from Gauss. To the same source of inspiration must be traced the almost simultaneous, but independent, researches of the Hungarian Wolfgang Bolyai and his son Johann. Gauss himself never published anything on the subject of non-Euclidean geometry; but we know from his letters to Schumacher that he had spent much thought on these questions, which had occupied him from his earliest youth, and had arrived at practically the same results as Lobachevsky and the Bolyais.

In the later development of non-Euclidean geometry and the closely related theory of n-dimensional spaces or manifoldnesses we find among others the names of Grassmann, Riemann, Helmholtz, Cayley, Klein, Lie; and in these the uninitiated may find a sufficient guarantee for the value of the subject.

In conclusion, a few words must be said of the present English translation. The original has been followed so faithfully that anybody possessed of an adequate knowledge of the Russian language will understand the translation very readily. The reading of such unidiomatic English is, however, exceedingly painful. Were it not for the direct statement on the title-page, we should never have ascribed this translation to Professor Halsted, whose vigorous command of the English language is well known. It seems almost incredible that a person whose native language is English should have written, or even passed in the proof, such sentences as these: (p. 3) "So in celebrating this day to Lobachevsky, we must remember with gratitude his teachers." (ib.) "His destiny was to be the teacher and protector not only of Lobachevsky, but of the scientist of our century most influential on the development of mathematics, Gauss. (ib.) "The mathematical ability of the boy-genius awakened the attention of the science-hungry Bartels." (p. 4.) " . . . he received the grade of 'Magister' July 10, 1811, for extraordinary advance in mathematics and physics." (ib.) " . . . the question of the lowering of the grade of a two-termed equation . . ."

The transliteration of Russian names is faulty and inconsistent; thus we find Pouchkin for Pushkin, Demidef for Demidov, Karamzen for Karamzin, Simenov for Simŏnov, etc. It is inconceivable why the name of the well-known astronomer Littrow should be persistently misspelled Lettrov. On p. 1, for 'November 9, 1807' read 'January 9, 1807.' The statement in the preface, p. vii., that "in 1500 Copernicus was enjoying the friendship of Regiomontanus and fulfilling with distinction the duties of a chair of mathematics" is singularly incorrect. Regiomontanus died in 1476, when Copernicus was three years of age; and, although Rhaeticus, in speaking of the residence at Rome in 1500, refers to Copernicus as 'professor mathematum,' it is now, in the absence of any direct evidence, generally accepted that the author of the *De revolutionibus* was never connected as teacher with any scientific institution.

ALEXANDER ZIWET.

UNIVERSITY OF MICHIGAN.

Laboratory Exercises in Botany, designed for the use of colleges and other schools in which Botany is taught by laboratory methods, by EDSON S. BASTIN, Am. Professor of Materia Medica and Botany and Director of the Microscopical Laboratory in the Philadelphia College of Pharmacy. Philadelphia. 1895. $2.50.

In a review of this volume it should be considered for whom it was written and from that standpoint an estimate should be made whether the purpose has been really accomplished. Being designed for students who are beginners, it leads them from the simple to the complex, and does it, we think,

in a very satisfactory manner. As a laboratory guide the work is perhaps a little too voluminous, 540 pages. It is divided into two portions, the first requiring work with the simple microscope, and consists of a series of lessons inductively arranged, which leads the student from a study of the root through the types of the largest families to a study of the seed and embryo. They are designed to give to the student a familiarity with the various forms, without burdening him with the technical descriptive terms, which are, however, summed up in tabulated plates for reference. The full-page illustrations of the first portion are numerous, very simple, excellently drawn and well printed.

The second portion of the volume, 270 pages, on vegetable histology, opens with a chapter on the compound microscope and the use of micro-chemical reagents, and is accompanied by excellent and practical tables of reagents and stains. The purpose of this volume limits its scope. It makes a good working guide to put into the hands of students who can give but a limited time to the study, but further than that, as a work upon vegetable histology, it is meagre.

The arrangement of this portion of the work is less commendable than the first. Its numerous illustrations can be classed as most good, few bad and a number indifferent, in general the simple elements of tissues being good, whereas those showing the tissues themselves, especially the more complex ones, are less to be approved.

The work is one which is admirably adapted for the use of students in pharmacy, for which it was probably first intended, and in the hands of a guide whose methods were similar to those of the writer, we conceive it to be excellent. In general its scope is limited; it gives facts but fails, we think, to point out those logical sequences of growth and development that lead the student to a rounded conception of the science of botany; it nevertheless is by far the best laboratory guide we have seen for directors of laboratories who wish to give their students a practical elementary knowledge of botany.

S. E. JELLIFFE.

Principles and Practice of Agricultural Analysis.—BY HARVEY W. WILEY, Chemist of the U. S. Dept. of Agriculture.—Easton, Chemical Publishing Co., 1894. Vol. I.

We have already called attention to the first part of this admirable work, now being published in monthly installments by the Chemical Publishing Company, and need not again speak of its general excellence of plan. If any fault is to be found with the work it is with its limited title, which is rather apt to mislead some into a supposition that the book will be of service only to the analyst, and as a laboratory manual alone. The twelve parts which have now appeared, nearly 600 pages in all, indicate a work of much broader scope, one which no scientific library can afford to omit from its catalogue. Of the first of the series we have already spoken. In No. 2 the subject of soils and soil formation is continued, the action of earth-worms, bacteria, air, etc., the qualities of the various soils and the discussion of certain peculiar soil types. An interesting chapter on sampling follows, and here is discussed in principle and practice all of the accepted methods now in use in various countries and among the leading workers in agricultural science. The study of the physical properties of soils and the description of methods of mechanical and microscopical analysis, etc., occupies some 200 pages, while the methods of chemical analysis, begun in No. 7 of the series, extends to the present issue. We know of no other work approaching the present in completeness and scientific value. The exhaustive treatment of the subject leaves nothing to be desired, and it would be difficult indeed to criticise any of its features. At the end

of each part is a Bibliography of works cited, and an inspection of these lists at once indicates the labor entered upon by the author, as well as that saved to those who have now the benefit of his research.

PHILADELPHIA. CHARLES PLATT.

Nitrogen and Water, or the Water Atoms and Their Relations. Part—The Earth's Atmosphere, by WILLIAM COUTIE.

The author of this polygraph of 31 pages is good enough to assure us that some things remain undiscovered, or at any rate we infer this to be his meaning. To discover the real meaning of many of his sentences would require the application of the calculus, since his thoughts soar off into space in what are apparently curved lines. It is probable that minds of the earth, earthy, like that driving this pen, are incapable of fully grasping the mighty thoughts here set forth. They are certainly startling and go to the root of all things.

It appears that we have all been mistaken in our conception of the design of Creation, at least those who have ventured to form any such conception have been mistaken. The real reason is thus set forth:

"It is evident that it is the law of change that gives the Creator some work to do and something that is new in all time. It is thus to Him the most important of all, for it is to Him preëminently omnipresent, universal and in all things forever new, and without it time would be a monotony and a burden, almost everything would be old and He would have nothing to do."

The following whack at our biological brethren is commended to their attention; their disgraceful Darwinian tendencies make it deserved, if somewhat severe:

"If we now turn to the results in time we find that, first, horse in our knowledge was of the size of a fox and walked on his heels. Now all horses of every kind walk on the point of their longest toe, and they are all many times the weight of a fox. Now, why did all horses get on their toes at the same time, or how did they get on the tips of their toes at all? Darwinism is to me a compound of utility and economy. But by what process of economy or utility did horses get on the point of their toes? To me, it is evidently the exclusive result of their Maker's will, and that the creation and government of the universe is an absolute despotism in all things."

This facer ought to settle the Darwinians; lest it should not, we subjoin another extract of like tenor:

"I found that a butterfly is an insect ornamented by scales, and that they are divided into day flies and night flies, and again divided into six thousand day or butterflies and sixty thousand night or moth flies, and that butterflies are purely and exclusively (so far as they are butterflies) things made for beauty by an agent or Maker who sees beauty of colors in the night, for there are sixty thousand kinds of night flies and only six thousand day flies. This led me to the undoubted belief that Darwinism applied to butterflies is worse than an error, for it leaves out the most important and essential part of the whole, which is, that the origin of species is the direct exclusive result of an intelligent design."

To the initiated the following will perhaps explain how some of Mr. Coutie's results were obtained:

"As the ways of this argument are so far from the ordinary beaten paths, my intent when writing it was to print in full along with it Newton's four rules of reasoning, pages 384 and 385, *Principia*, to show that this is in full and exact accord with them."

"This design led to a full, careful review of the men, their method and their particular results, that I found that these rules are wholly insufficient for my purpose. They are perfect for his purpose, but insufficient when applied to this paper."

This, so far as we are able to understand it, looks black for Newton.

Among other gems of style and statement, we have the following:

"The history of origin leads us far back into the distant past."

"What this subject learns from this observation of the heavens is that the same rules that govern the atoms."

"The density of the air is the result of its own weight."

The author has also discovered a few less important matters of detail. Among other things two new—what shall we call them; not elements for they are, according to our present notions, compound. The first of

these new somethings is kirs. This is no common mangy kirs, but a new kind of kirs altogether. He or it—for the author says enough about the relations of the atoms to make one careful—is introduced to our notice as follows:

"The most resultant discovery of all is that kirs is a hydrate of nitrogen, having the atomic form N_3HN_3."

The second something new is Stuart, which is N_3H, it seems. According to the author this, as well as kirs, is unobserved. We understood that Curtius not many years since discovered a compound having the symbol of Stuart, but this is perhaps a mistake. Carbon has been found to be AN., ice is Aq. and made up of Stuart, Cyanogen and more Stuart. Coke equals kars and A. We are nowhere informed what is meant by A, nor is it easy to see what difference there is between 'combining constituents' and 'constituents' except with the eye of faith. The author explains, however, that "The grand difficulty of the calculation is that the revelations at the end constantly contradict the premises at the beginning."

Everything about this wonderful pamphlet is new, even the spelling is sui generis. For example: Flourine, Glucium, Rubedium, Phosphorous, Telerium, Tantalium, Lanthanium, Paladium.

We hope that E. H. Lisk, printer, Troy, N. Y., turned off a large edition of these pamphlets. They will all be needed, and when obtained ought to be carefully preserved as an illustration of the magnificent reach sometimes attained by the American intellect.

EDWARD HART.

NOTES AND NEWS.

BIOLOGY.

THE Tenth Annual Fish Commissioners' Report from Michigan is entirely in the field of fresh-water biology. It is important to mark the rapid development of biological work in the central universities of this country, and to note that the work carried on by the State is so largely by the coöperation of the biologists of the University. Thus two of the papers of this report are by Professor Jacob Reighard, the first being a study of the development of the wall-eyed Pike, the second a valuable résumé of the whole subject of artificial fertilization. The Bulletin, No. 4, of the Commission, which we receive at the same time, contains a preliminary account of the biological examination of Lake St. Clair during the summer of 1893. This was suggested by the continued decrease in the number of Whitefish, but very wisely the work extended over a broader field. The objects of this examination are stated as follows: "(1) To study carefully and in the broadest possible way the life in the lake. After examining the physical characteristics of the lake, such as the color, transparency and chemistry of the water, a study of this sort should include a determination of the kinds of animals and plants in the lake. Every species should be sought out, carefully described and figured, and a specimen of it preserved. Then the habits of each species should be known, its habitat, its food, its enemies and its parasites. The numbers of animals and plants of each species in a given volume of water should be determined and the variations in these numbers in different parts of the lake and at different seasons of the year. Such a collection of data would form a complete picture of the biology of the lake." The work was under the direction of Professor Reighard, assisted by Dr. Ward, of the University of Nebraska, by Mr. Frank Smith, of the University of Illinois, and by several assistants from the University of Michigan. The materials collected were widely distributed for determination, and the reports are by Dr. Blanchard, of Paris, Dr. E. A. Birge, of the University of Wisconsin, and others. The survey seems to have been carried on with all the thoroughness both

in the collection of littoral, pelagic and deep-lake types, which characterizes the best marine work, and the final results promise to be of the greatest interest and importance.

Mr. Arthur Bibbins, who has been engaged during the past year in investigating the fauna of the Potomac Formation, in the interest of the Woman's College of Baltimore, has made a considerable collection of reptilian remains, mostly from the vicinity of Muirkirk, Md. The specimens represent the four species of Dinosaurs described by Professor Marsh under the names of *Allosaurus*, *Pleurocœlus* and *Priconodon*. A tibia, probably that of *Allosaurus*, measures 10 inches in width and 32 inches in length, although the ends are lacking. A single tooth seems to be referable to *Astrodon Johnsoni*, Leidy, which was based on a tooth found at Bladensburg, Md. The conditions are very unfavorable for collecting, as the specimens occur in a tough clay, often at a considerable depth, and are much scattered.

Dr. S. W. Williston, of Lawrence, Kansas, has in press a work, entirely rewritten, on the classification and structure of North American Diptera. It will contain tables of all the North American genera, including those from Central America and the West Indies, together with descriptions of larvæ, habits, anatomy, etc. It will appear next autumn. In its preparation he has had the assistance of Messrs. Aldrich, Townsend, Snow and Johnson, who have kindly prepared or revised the tables of the families with which they are best acquainted.

At the second open meeting of the Royal Society, on February 28th, Prof. W. F. R. Weldon opened a discussion on variation in animals and plants, his remarks being based on the report of a committee, consisting of Mr. Francis Galton, Mr. F. Darwin, Professor Macalister, Professor Meldola, Professor Poulton and Professor Weldon himself, its object being to conduct statistical inquiries into the measurable characteristics of plants and animals. The first part of the report which was presented was described as 'an attempt to measure the death rate due to the selective destruction of *Carcinus mœnas* (the shore crab) with respect to a particular dimension.' Another paper bearing on the subject under consideration was presented by Mr. H. M. Vernon, on 'The Effect of Environment on the Development of Echinoderm Larvæ: An Experimental Inquiry into the Causes of Variation.' An interesting discussion followed, in which Mr. Thiselton Dyer, Professor Ray Lankester, Professor A. Agassiz, Mr. Bateson, Sir H. Howorth and the chairman took part. There seemed to be a prevailing doubt as to the suitability of mathematical methods in biological research.

Prof. H. W. Conn contributes to the March number of the *American Naturalist* an account of the Cold Spring Harbor Biological Laboratory, of which he is the director. The article is illustrated by four plates, showing the buildings and location. The laboratory was organized by Prof. F. W. Hooper as a branch of the Brooklyn Institute of Arts and Sciences, and held its first session in July and August, 1890, under the direction of Dr. Bashford Dean, now of Columbia College. The Cold Spring Laboratory does not rival the Wood's Holl Laboratory in the amount of research work accomplished, but offers exceptional facilities for students requiring instruction.

APPROPRIATIONS FOR THE U. S. GEOLOGICAL SURVEY.

The appropriations for the U. S. Geological Survey for the fiscal year 1895–96, as made by Congress at its last session, will enable the bureau to continue its work under favorable circumstances. The appropriations for topography, geology, paleontology and chemistry are the same as those

for the present year, except that in the case of geology there is an additional appropriation of $5,000 for the specific object of the investigation of the gold and coal resources of Alaska. For the rest, there is an appropriation for the preparation of the report on the mineral resources of the United States of $18,000, an increase of $3,000; and further was inserted in connection with this work, under the head of Public Printing and Binding, a clause providing for the printing of advance copies of papers on economic resources, and for this work an appropriation of $2,000 was made. Under the head of engraving and printing the geological maps of the United States, authority was granted the Director to sell copies of topographic maps, with a descriptive text, at cost, with ten per centum added. The object of this item is to provide for the preparation of a series of ten or more maps, with text, to illustrate the typical topographic features of the United States, for use principally in teaching. It is anticipated that the maps and text will be prepared during the summer. To the appropriation for 'gauging the streams and determining the water supply of the United States, including the investigation of under-ground currents and artesian wells in arid and semi-arid regions,' $7,500 was added, making the appropriation for this work $20,000.

The total appropriation for the Survey, including all field and office expenses and salaries, is $515,000.

An appropriation of $200,000 was made for a survey of the lands of the Indian Territory, with the provision that the "Secretary of the Interior may in his discretion direct that the surveys in the Indian Territory, herein authorized, or any part of them, be made under the supervision of the Director of the Geological Survey." This work will result in the making simultaneously of a land subdivision survey and a topographic map.

GENERAL.

THE German Anthropological Society is publishing an extensive description of the anthropological collections of Germany. Sixteen parts (costing from 2–15 M.), prepared by competent authorities, have already been issued.

THE *Technologisches Wörterbuch*, edited by Gustav Eger and published by Vieweg, Brunswick, is a full English-German and German-English dictionary of scientific and technical words, which should have as large a sale in America as in Germany.

THE first volume of the memoirs from the Department of Botany of Columbia College, a monograph of the *North American Species of the Genus Polygonum*, by John K. Small, is now in press.

DR. ERNST MACH, Professor of Physics in the University of Prague, has accepted a Professorship of Philosophy in the University of Vienna, and will direct a Laboratory of Experimental Psychology.

PROFESSOR E. W. HOPKINS, of Bryn Mawr College, succeeds Professor Whitney in the chair of Sanskrit and Comparative Philology, and Professor E. G. Bourne, of Western Reserve College, has been elected Professor of History, at Yale University.

PROF. WEIERSTRASS, of Berlin, has been elected Foreign Associate of the Paris Academy of Sciences; he received forty-three votes, one being given to Prof. Frankland and one to Prof. Huxley.

PROF. E. DORN succeeds Prof. Knoblauch as Director of the Physical Laboratory of the University of Halle.

PROF. M. K. RÖNTGEN, of Würtzburg, has been called to the chair of Physics in the University of Freiberg, vacated by Prof. E. Warburg.

DR. R. BRAUNS has been made Professor of Mineralogy in the University of Tübingen.

DR. A. KOSSEL has been made Professor of Physiology in the University of Marburg.

DR. K. BOEDEKER, Professor of Chemistry in the University of Göttingen, died on February 22d, aged seventy-nine years.

SIR WILLIAM SAVORY, an eminent surgeon, and at one time Professor of Comparative Anatomy and Physiology at the College of Surgeons, died on March 4th, at London, in his sixty-ninth year.

DR. GEORG VON GIZYCKI, Associate Professor of Philosophy in the University of Berlin, died early in the present month.

DR. DARWIN G. EATON, formerly Professor of Natural History in Packer Institute, died on March 17th, at the age of seventy-two years.

PROF. PETER H. VANDER WEYDE, editor of *Manufacturer and Builder*, and formerly Professor in Girard College and at the Cooper Institute, died at New York, on March 18th, at the age of eighty-two years.

DR. HENRY COPPÉE, Acting President of Lehigh University, Professor of English Literature in the University of Pennsylvania, 1855 to 1866, and President of Lehigh University, 1866 to 1875, died at Bethlehem on March 21st, at the age of seventy-five years.

SCIENTIFIC JOURNALS.

THE PHYSICAL REVIEW, MARCH–APRIL.

On the Attractions of Crystalline and Isotropic Masses at Small Distances: A. STANLEY MACKENZIE.

The Influence of Temperature upon the Transparency of Solutions: EDWARD L. NICHOLS and MARY C. SPENCER.

Determination of the Electric Conductivity of Certain Salt Solutions: ALBERT C. MACGREGORY.

The Apparent Forces between Fine Solid Particles Totally Immersed in Liquids, II: W. J. A. BLISS.

Minor Contributions; New Books.

THE AMERICAN NATURALIST, MARCH.

In the Region of the New Fossil, Dæmonelix: FREDERICK C. KENYON.

The Cold Spring Harbor Biological Laboratory: H. W. CONN.

Minor Time Divisions of the Ice Age: WARREN UPHAM.

The Skunk as a Source of Rabies: W. WADE.

The Classification of the Lepidoptera: VERNON L. KELLOG.

Recent Literature; Recent Books and Pamphlets. General Notes:—Geography and Travels; Mineralogy: Geology and Palæontology; Botany; Zoölogy; Embryology; Psychology; Archæology and Ethnology.

THE BOTANICAL GAZETTE, MARCH.

Apparatus for Physiological Botany (With plates IX.–XII.): W. C. STEVENS.

On the 'List of Pteridophyta and Spermatophyta of Northeastern America: B. L. ROBINSON.

Flowers and Insects, XIII.: CHARLES ROBERTSON.

Noteworthy Anatomical and Physiological Researches.

Briefer Articles; Editorial; Current Literature; Notes and News; Supplement.

NEW BOOKS.

Louisiana Folk-Tales. Collected and edited by ALCÉE FORTIÉR. Boston and New York, published for the American Folk-Lore Society, Houghton, Mifflin & Co. 1895. Pp. xi+122. $2.

The Free Trade Struggle in England. M. M. TRUMBULL. 2d Edition. Chicago, The Open Court Publishing Co. 1895. Pp. 288. 35 cts.

Beiträge zur Kentniss des Wesens der Säcular-Variations des Erdmagnetismus. LOUIS A. BAUER. Berlin, Mayer & Müller. 1895. Pp. 54. M. 3.

Field, Forest and Garden Botany. ASA GRAY. Revised and extended by L. H. BAILEY. New York, American Book Co. 1895. Pp. 519.